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## **The future of international political economy: Introduction to the 20th anniversary issue of RIPE**

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An anniversary issue provides an inescapably inviting opportunity to reflect on the past, evaluate the present, and contemplate the future. Eschewing the self-congratulatory rhetoric of traditional anniversary celebrations, we have devoted this 20th anniversary issue of RIPE to contributions that critically examine the academic discipline of international political economy, focusing on our collective challenges and limitations as much as on our achievements. As every author knows, it is the thoughtful, constructive, and above all critical review that ultimately pushes us to produce better scholarly work. The global financial crisis mandates such a reassessment, as did the fall of communism that birthed this journal.

John Hobson opens the anniversary issue with a two-part revisionist historiography of our field. In Part One, he extends the transatlantic 'British vs. American School' debate sparked by Benjamin J. Cohen (2007) to identify the Eurocentric metanarratives of classical and modern political economy. In Part Two, Hobson deconstructs the Eurocentrism of contemporary Open Economy Politics (OEP)<sup>1</sup> and proposes an alternative research agenda for what he dubs an 'inter-civilizational' political economy.

Catherine Weaver and Jason Sharman then take us on a quantitative exploration of the transatlantic debate. Using survey data from the Teaching, Research and International Policy (TRIP) survey, they analyze the evolution of IPE in international academic journals and curricula. More specifically, they identify RIPE's place within the discipline and the extent to which RIPE has fulfilled its founders' mandate to represent the theoretical and methodological diversity of IPE scholarship.

Moving beyond the evolution of IPE, in this issue's final research article Stephen Nelson and Peter Katzenstein argue that the recent global financial crisis demonstrates how fundamental uncertainty and financial instability remain central to the dynamics of modern capitalism. The crisis has posed a formidable challenge for students of IPE, which many have met by reassessing the relevance of past scholarship to our understanding of the present (e.g. Cohen, 2007; Helleiner, 2011; Moschella, 2010; Mosley and Singer, 2009). To be sure, IPE as a field is ill-equipped for prediction and does not consider this to be one of its main vocations (Mosley and Singer, 2009: 420). But the re-discovery of uncertainty and instability reminds us that scientific methods aspiring to transform true uncertainty into calculable risk cannot but fail to foresee the future. An excessive focus on time-invariant probabilities as the goal of IPE scholarship is at odds with a need for sensitivity to the historical ruptures that may creep up on us unnoticed if we take the highly contingent features that define any era for granted until it is over.

The global financial crisis may in time spur deep intellectual shifts in IPE scholarship, much as the collapse of communism in Eastern Europe and the Soviet Union promised to do two decades earlier. Those events helped to convince a group of visionary scholars in the United Kingdom to found a new journal, the *Review of International Political Economy*, dedicated to challenging existing intellectual orthodoxies within IPE. When RIPE published its first issue in 1994, the editorial board underlined the profound impact that the fall of communism could and should have on the social sciences:

We are only beginning to realize the extent to which the Cold War intertwined with the orthodoxies. What we have witnessed in recent years, and not just in Eastern Europe, has seriously shaken the orthodoxies, thus providing a window of opportunity to bring together analyses of former opposed positions (RIPE Editors 1994).

However, rather than enabling new dialogue between formerly opposed traditions of thought, in practice the end of the Cold War affected the broader field of political economy by, first, appearing to vindicate liberal economic thought and, second, by making previously lively

debates feel dated and of little use. Within IR, political economy was relegated to the backseat. The events of 9/11, which gave a sense of urgency and primacy to security studies, only reinforced this trend. Moreover, in parts of IPE, notably in what Cohen has described as the 'American school', scholars have sought to standardize inquiry in a positivist and micro-analytical fashion. A review of the field in 2002 underlined proudly that scholars have 'approached consensus on theories, methods, analytical frameworks, and important questions' (Frieden and Martin, 2002: 118).

In this introduction to our 20th anniversary issue, we want to re-emphasize and re-frame our founders' insistence on questioning moves towards orthodoxy, consensus, and closure, particularly in light of the global financial crisis. Indeed, as the lively debate about differences between American and British approaches to IPE demonstrated, it is always wise to be wary of apparent consensus and intellectual monocultures (Germain, 2009; Higgott and Watson, 2007; Keohane, 2009; Maliniak and Tierney, 2009; McNamara, 2009). This debate brought to the fore age-old cleavages in the study of social life: epistemological and methodological questions, structure-agency debates, disagreements about the utility of de-contextualized and ahistorical analysis, the merits and demerits of parsimony, the desirability of a normative basis underlying IPE research, and the importance of the practical 'usefulness' of research.

We continue to believe that these are core issues for scholars to confront directly. At the same time, as a focus of IPE scholarship, these debates have at times turned academic in the negative sense of the word. Any reasonably open-minded IPE scholar recognizes that there is no one 'right' way of doing IPE, if only because every way of generating knowledge about the world we live in has severe limits. We argue below that the global financial crisis has underscored the importance of embracing what we call a 'pragmatic IPE', by which we mean an IPE that responds to real-world developments and recognizes that complex examinations of multi-faceted phenomena demand a commitment to methodological pluralism and engagement across disciplines.

### IMPLICATIONS OF THE GLOBAL FINANCIAL CRISIS FOR IPE RESEARCH

In the past 20 years RIPE has published insightful arguments on the fall of communism and economies in transition, World Bank policy and internal politics, the emergence of the World Trade Organization and the new regionalism of trade and investment treaties, the international monetary system and the International Monetary Fund, and far beyond, without favouring any one voice, theoretical approach, or methodology. RIPE articles have been written by political scientists, sociologists,

geographers, and economists, and have featured research ranging from realist analyses of monetary integration, to institutionalist assessments of international financial institutions, to constructivist perspectives on how powerful interest groups use certain ideas to reframe issues for political gain.

That said, the crisis has laid bare some fundamental challenges for all contemporary IPE scholarship. IPE researchers working in finance (particularly those influenced by Keynesian and post-Keynesian traditions in economics) recognized the inherent vulnerabilities and power disparities embedded in liberalized, internationally integrated, and opaque financial markets (e.g., Helleiner, 2011; Blyth, 2002; Best, 2005; Cohen, 1998; Kirshner, 2003, Abdelal, 2007). But the depth and virulence of the crisis caught most scholars by surprise, notably if we consider research published in the most highly ranked international relations journals. Indeed, Manokha and Chalabi (2011) found that the majority of prominent international relations journals did not publish a single article on the financial crisis in the years 2008–2011.

In particular, the challenges that the crisis presents to contemporary quantitative IPE are greater than its practitioners concede (Leblang and Pandya, 2009: 430–35; Mosley and Singer, 2009: 420–429; Lake, 2011: 45–52). How can we reconcile the assumption that social reality is structured by comprehensible mechanisms, presumed to produce patterns that make them recognizable, with the existence of sudden ruptures such as the crisis? The standard answer, compatible with much quantitative IPE scholarship and the neoclassical economic models from which it takes inspiration,<sup>2</sup> is that exogenous shocks can disturb the mechanisms that otherwise gravitate towards stability, be they social, political, or economic. More sophisticated versions of this argument point to the existence of multiple self-sustaining equilibria, shifts between which can be rapid.<sup>3</sup>

Yet whether or not such a view of IPE is plausible in general, it faces a significant challenge if the shocks that it construes as exogenous are so momentous that they significantly disrupt at least some of the patterns that have hitherto characterized the global economy. Does what we thought we had learned about the politics of diverse issues such as domestic and international inequality, development, trade and investment patterns, aid politics, and democratic and autocratic politics in the shadow of globalization still hold in a post-crisis world? Maybe yes, maybe no. For us, answering that question is an empirical as much as a theoretical challenge. But it is clear that much quantitative IPE scholarship has seriously underestimated the historical contingency of the economic and political realities that it analyzed.

Cohen (2009: 436–444) and others have claimed that this research has necessarily missed the crisis because the tools it employs have systematically excluded events of the credit crisis-order from view. We have been

witnessing one global crisis, not dozens of isolated national ones that would readily lend themselves to large-n analysis (Oatley, 2011). Much quantitative pre-crisis IPE scholarship had no crisis-expectation because it takes ‘initial equilibrium [as a] starting point. [...] The underlying paradigm of neoclassical economics emphasizes comparative statics rather than dangerous dynamics’ (Cohen, 2009: 442). The method emphasizes stability or incremental change because the patterns it seeks to uncover are effectively a-historical. As Pevehouse and Brozek (2008: 465) have argued:

The vast majority of statistical models that are published in political science [...] posit a host of restrictions. Many of these assumed restrictions, however, are untenable, such as assuming that all explanatory variables are exogenous, lag structures are one period, and coefficients are stable across the entire estimation sample.

The three problems Pevehouse and Brozek identify all apply to common understandings of the crisis. Lag structures can vary enormously (i.e., when do factors that contribute to the build-up of crisis actually have an observable effect?). Coefficients may not only vary over time, but they may also actually change sign – for instance, the accumulation of public debt may promote or undermine economic growth. Factors that contribute to financial stability at one point in time may erode it at another. Advanced statistical techniques such as autoregressive integrated moving average (ARIMA) or autoregressive conditionally heteroskedastic (ARCH) models may improve upon our ability to model these diverse effects. But even these techniques are unable to capture the complex dynamics of the crisis. As we have seen, the apparent relative stability of the pre-crisis decades was unsustainable, and it also generated conditions that enhanced the severity of the subsequent crisis by inducing excessive confidence and optimism while simultaneously creating intensely vulnerable financial superstructures (Akerlof and Shiller, 2010; Farhi and Tirole, 2012; Minsky, 2008 [1986]; Rajan, 2011; Skidelsky, 2011). Income and wealth inequality, wage compression, slow growth, global and regional imbalances, and the power of the global financial community during the ‘Great Moderation’ also contributed to the creation of the ‘light touch’ regulatory systems and fragile financial structures and financing arrangements that culminated in the crisis (Baker, 2009; Crotty, 2009; MacEwan and Miller, 2011; Stiglitz, 2010; Tabb, 2012; Wade, 2008).

More broadly, the economics profession has also been affected by the crisis. Not least, interest in the findings of behavioral economics is growing (e.g., Ariely, 2013; Kahneman, 2011), especially in regards to the behavioural and neurobehavioural aspects of financial market actors

(e.g., Coates, 2012), and there is new attention to the ways that the economics profession itself may have contributed to the crisis (e.g., Colander *et al.*, 2009; DeMartino, 2010: ch. 10; Hodgson, 2009; Krugman, 2009; Mirowski, 2010a, 2010b; Stiglitz, 2009).

The understanding of the crisis that thus emerges might be described as contingent stability. Stable periods and phenomena are amenable to quantitative treatment because they concern the systematic reproduction of patterns over time. Contingencies, in contrast, are researchable through qualitative methods at best because they are either incidental or structural – and in any case come in such small numbers that statistical treatment is not an option.<sup>4</sup> Understanding history as alteration between stability and change, with more or less abrupt ruptures, is fully compatible with diverse yet historically sensitive landmark works in political economy. While Charles Kindleberger's *The World in Depression* spawned the 'hegemonic stability theory' that so influenced international relations as a field, it is Kindleberger's *Manias, Panics, and Crashes* that represents the historical cornerstone for understanding the political economy of financial crises. Other work, such as Carr's analysis of the 20 years' crisis, the oeuvre of Gramsci and Polanyi, and more recently Frieden's *Global Capitalism: Its Fall and Rise in the Twentieth Century* (2006) should be required reading for all IPE scholars before conducting applied research on more recent events.

### TOWARDS A 'PRAGMATIC IPE'

Where do we go from here? Debates about research methods challenge us to reflect on the inferential costs of any research strategy we choose – something we too often ignore. But as our experiences with the crisis demonstrate, they are no excuse for not taking the real-world problems 'out there' by the horns and using holistic and interdisciplinary perspectives to tackle them. Seen from that angle, IPE research has matured in recent years. In previous decades, scholars invested considerable energy first to 'prove' that political economy mattered to international relations, then that (historically grown) institutions mattered to IPE, and finally that ideas mattered, too. Then leaving behind (for the most part) these narrow debates, scholarship asked under which circumstances, in which ways, and to what degree ideas or institutions mattered, and again invested considerable effort in generating increasingly fine-grained answers to these questions (e.g. Milner, Dobbin and Garrett, 2006). A key lesson from this work is the importance of contingency: because no single theoretical perspective offers exclusive purchase on reality, asking in abstract terms which perspective is 'right' is not very enlightening. We have learned (again) that what holds today may not hold tomorrow, what holds in France may not hold in Finland, and what holds in finance

may not hold in trade. Add a few more dimensions of complexity, and modelling the real world in a way that still generates dependable patterns and causal relations becomes a difficult, if not impossible job.

To our minds, these points argue in favour of a pragmatic IPE. Pragmatic IPE accepts that scholars in the IPE community employ and advance different kinds of methodological tools but also remembers that these tools are just that – tools – and not ends in themselves. The ends are contributions to debates about real-world problems that matter (Keohane, 2009; Lake, 2010). One of the perennial attractions of IPE as an inter-discipline is the irreverence with which its practitioners have claimed exciting and relevant developments in global society as objects of analysis. Money laundering, land-grabbing, water politics, and struggles over access to life-saving medicines are fair game for IPE scholars just as much as the twenty-first-century marriage of capitalism and state-led development in China or the transformation of the global economy through digital technology. If anything, the panoply of real-world topics that IPE claims as its home turf is so broad that the greater challenge is to identify those that may merit particular attention or for which IPE approaches are particularly well placed to provide incisive analyses – a point to which we return below.

If pragmatic IPE is marked by a focus on real-world problems rather than on theory for its own sake, a rejection of any separation of politics from ‘the economy’, an awareness that local and global developments cannot be analyzed in isolation from one another and, in consequence, an acknowledgement of the complexity of the world we study, we must accept that there are limits to how watertight our analyses can ever be. In the end, whatever its scientific aspirations, IPE remains an informed conversation by scholars who care and know about similar issues and take both inspiration from and issue with each other’s readings of the world.

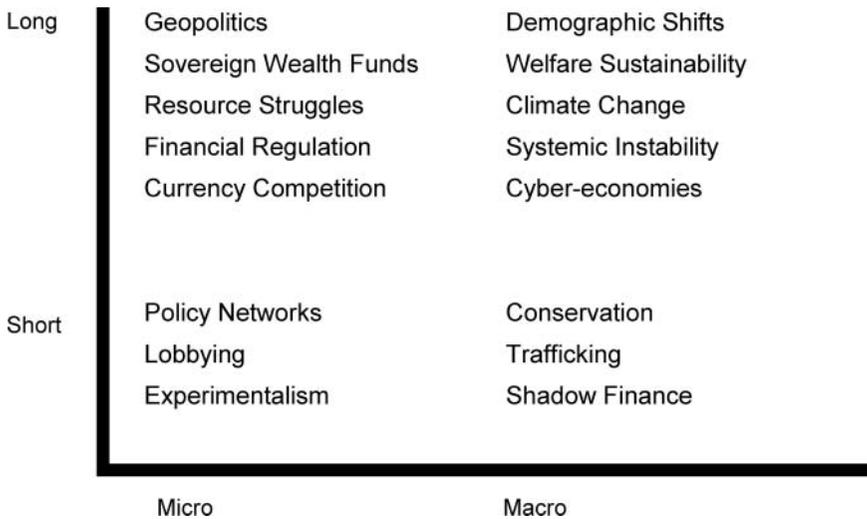
Such an approach suggests a middle road between what in earlier debates has been stereotyped as British and American IPE. With Keohane (2009), we cherish the normative aspirations of the former and the eagerness to adjudicate between competing claims of the latter. An empirical grounding remains essential to that mission and is the core to works from scholars that Ravenhill (2009) characterized as the ‘missing middle’ but which actually comprise much of the rank and file of IPE scholarship in many parts of the world (see Sharman and Weaver, this issue). Pragmatically, the question is not whether, for example, constructivism or post-structuralism is ‘right’, but what insights each perspective has to offer on specific issues in IPE that can withstand informed criticism. That does not obviate the need for theory-focused discussions (e.g. Bieler and Morton, 2008), but IPE should avoid getting stuck in methodological or epistemological navel-gazing while the world keeps on turning

and offering up real problems and questions that demand our intellectual attention (cf. Friedrichs and Kratochwil, 2009).

**A SUBSTANTIVE IPE RESEARCH AGENDA**

Espousing pragmatic IPE means being attentive to substantive problems that influence debates at the intersection of politics and economics and beyond national boundaries. This suggests that as a collective, IPE must address both practices at the micro-level – regardless of whether they concern, for example, gendered division of labour in average households or patterned interactions among policy elites – and macro-level changes such as structural transformations in trading patterns in the world economy. We should also be attentive both to long arcs of history and to the short-term developments that actually animate the social, political and economic actors we study. This is a tall order indeed.

Figure 1 provides illustrative examples of research topics plotted according to their predominant time horizon and potential methods of discovery (that is, a focus on micro-level dynamics or macro-level phenomena or, more traditionally ‘agents’ and ‘structures’). Some of them are well established in the IPE literature; others have hardly been touched upon but are apt to provide important future challenges. Ever since Susan Strange’s (1994) call for ‘catholic complexity rather than protestant parsimony’ in response to the narrowing of the field that sparked



**Figure 1:** Illustrative IPE Topics by Time Horizon and Focus.

the birth of the journal, methodological diversity has been praised in the pages of *Review of International Political Economy*. Following this tradition we suggest that different kinds of problems can be tackled by a range of methods that themselves have a micro or macro focus. For example, we suggest that many emerging issues of inquiry may be best addressed with methods that have entered IPE scholarship more recently; for example, network analysis (Henriksen, 2014; Young, 2013) or experimental methods that target micro-level behaviour with macro-level consequences (Findley, Nelson, and Sharman 2013). We also acknowledge that theories and methods are not one and the same, but that tackling empirical problems requires consideration and reflection on ontological, epistemological, theoretical, and methodological choices. Of course, these considerations are interlinked and reflect different starting positions, training, and skill sets. The key here is that reflections on how to address problems that present different time horizons and questions of scale have long been part of *Review of International Political Economy* and will continue to be so.

Starting from the top left and moving clockwise, we have identified long-term topics such as geopolitics, transnational financial regulation, currency competition, and resource struggles as often best suited to study through micro-focused methods. Unraveling them is a micro-scale concern inasmuch as it will be key to identify the often limited number of relevant actors and understand their (perceived) interests. The time frames in which the relevant politics unfold, as well as their implications, are long rather than short-term. Similarly, tracing individual interests and strategies in regards to sovereign wealth funds pose important questions of how to theorize new practices and mechanisms of great power politics (Blanchard, 2011). For such long-term topics, we envisage that conventional methods such as process tracing will continue to be fruitful. Bringing the political aspect of these IPE issues into the spotlight is particularly important in order to broaden analysis beyond an OEP framework.

In the top right quadrant we find long-term topics such as demographic shifts, climate change, and welfare state sustainability that can be fruitfully addressed with macro-focused methods. Scholarship on welfare state sustainability has included American work on retrenchment and European and British research on discursive and institutional structures that permit enduring forms of neoliberalism (Schmidt and Thatcher, 2013). Investigating long-term phenomena like climate change and demographic change naturally require large datasets. These topics can also be studied through a combination of macro- and micro-focused approaches, such as tracing the cultural and political forces that place the car at the centre of modern capitalism and how it is embedded in everyday life (Paterson, 2007), or following macro trends on demographic change and professional activity in order to combat its perceived

problems (Seabrooke and Tsingou, 2013). The reality of systemic instability prompted by factors as diverse as the rise of China and economic innovation (both in terms of technologies and processes) should keep us on our toes as well.

In the bottom right corner we situate short-term topics that can be addressed with more macro-focused approaches. We have named conservation, trafficking, and shadow finance as examples. Recent work on conservation has identified harm through tourism and located it within broader logics of neoliberalism (Duffy, 2013). On human trafficking, new research has used panel data to provide a quantitative assessment of the differential impact of social and economic globalization on women's rights during the past two decades, highlighting how economic globalization, by itself, does little for rights for trafficked women, and that improved conditions require longer-term changes in social attitudes and perceptions (Cho, 2013). Early studies of shadow finance markets involving phenomena such as remittances, non-state currencies, and 'offshore' banking have identified how to locate these activities within more conventional structures of financial capitalism (Palan and Nesvetailova, 2013).

Finally, the bottom left corner is occupied by short-term topics that can fruitfully be studied with micro-focused approaches. In this quadrant we stress the importance of examining policy networks, lobbying, and the creation of deliberative forums, as depicted for example in the Experimentalist Governance literature (Sabel and Zeitlin, 2010). To map links between actors, social network analysis is an important tool to deploy (van Apeldoorn and de Graaf, 2012). Elite interviewing is vital to understanding how those involved in policy networks and lobbying view their interests, mandates, and themselves, such as belonging to a profession, institution, or network. One exemplary methodological innovation is to trace policy in international organizations to team composition, linking the micro phenomenon – the team working on a particular issue – to broader policy implications (Chwieroth, 2012).

## THE NEXT TWENTY YEARS

We neither can nor would want to predict all areas of inquiry that will come to the attention of IPE practitioners and scholars in the years to come. That said, the rise of emerging economies will inevitably shape future IPE inquiry, as will concerns surrounding sovereign debt and global imbalances, the social and environmental impact of international economic activities, the rise and governance of technologies such as the Internet or biofuels, and ever-evolving class, gender, and ethnic interactions and inequalities. Many other issues could be added to this list, and many in fact engage both short and long-term phenomena at multiple

levels of analysis. Our micro/macro and long/short plotting exercise is not intended to put real-world problems in neat boxes once again. Instead, it illustrates how moving beyond agency-structure debates can give us a clearer sense of the empirical consequences of the problems on which we focus our research. Resolving theoretical debates whose contributions remain entirely abstract will do little to illuminate pressing issues for people affected by the international political economy in their everyday lives.

As reflected in our plea for a 'pragmatic IPE', RIPE champions scholarship in which concrete, relevant research questions drive methodological choice and theoretical approaches. Different research questions demand different methods and interpretations. For example, a question such as 'Does the IMF still mandate conditionality programmes?' could comprehensively be answered in a large-n statistical analysis of countries with and without IMF country programmes combined with parallel case study analysis for verification. On the other hand, a question such as 'Do new economic theories about financial stability drive decisions in the IMF?' can only be answered through systematic interviews with IMF decision-makers. RIPE has always welcomed diverse analyses, as long as the research questions are sharp and they are answered with the appropriate methodological tools. On this 20th anniversary of RIPE, we eagerly look forward to extending the reach of pragmatic IPE, to preserving the journal's role as a forum for open-minded and lively scholarly debate, and to uphold its proud Hirschmanian tendencies to trespass across disciplinary boundaries in ways that illuminate dynamic, complex, and ever-changing social phenomena.

## NOTES

- 1 Lake (2009, 225) summarizes OEP as follows: 'OEP begins with individuals, sectors, or factors of production as the units of analysis and derives their interests over economic policy from each unit's position within the international economy. . . Analysis within OEP proceeds from the most micro- to the most macro-level in a linear and orderly fashion, reflecting an implicit unidirectional conception of politics as flowing up from individuals to interstate bargaining.'
- 2 This scholarship often has an underlying assumption that social processes tend towards order in some form and hence stability, meaning that the reproduction of the status quo is more likely than is significant change. This concerns relative stability in prices, political systems, economic organization, social mores, etc. Turmoil is the exception, not the rule.
- 3 For example, both liberal democracy and clientelistic authoritarian regimes might be self-reproducing. Of course, recent events in the Maghreb have dented this view in the same way the implosion of the USSR dented international relations orthodoxy two decades ago. We tend to stick with observable

trends, however, and political stability is visible while it lasts, whereas the rot of a political order from within may not be.

- 4 Eichengreen seconds this dichotomy when he writes that ‘the alternative to theory is history’ (Eichengreen, 2009: 58). But history is not without its own dangers. It is a biased guide to the future, providing a very selective reading of the present. For example, as head of the Federal Reserve Ben Bernanke seems to have relied heavily on his own interpretation of the Great Depression when formulating his policy response to the credit crisis. The jury is still out on both whether this medicine has cured the patient and whether the two episodes were really comparable in the ways he presumed.

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